

*THE ETERNAL ANTITHESIS: A COMMENTARY ON  
DONAHOE, PALMER, AND BURGOS*

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We are concerned with the eternal antithesis between the two inseparable components of our knowledge, the empirical and the rational. (Einstein, 1979, p. 310)

Some years ago I built a large demonstration pigeon chamber that was constructed of transparent Plexiglas on three vertical sides and the top. The intelligence panel and the bottom were opaque. The chamber was placed in a small room in the laboratory, and a pigeon was trained to peck a transilluminated key in the usual manner with presentations of a few seconds access to grain. Key pecking was quickly established, and the schedule of reinforcement was changed gradually from fixed-ratio (FR) 1 to FR 50. Half-hour sessions were conducted daily for a week or so, at which time responding seemed stable. For reasons I no longer recall, I decided that the chamber was not in the most appropriate orientation, so one day I simply rotated it 180 degrees. I then placed the pigeon as usual in the chamber, closed the door to the room, and started the program. When I returned a while later, I noticed that no responses had occurred. Upon opening the door, the reason was obvious: The pigeon stood in its usual location facing directly away from the key panel! A bit of further training was necessary to reestablish key pecking on the opposite wall.

As Donahoe, Palmer, and Burgos emphasize, "context sets the occasion for responding, although its influence may not be apparent until the context is changed" (p. 196). More generally, Donahoe and Palmer (1994) asserted, "what is selected is always an environment-behavior relation, never a response alone" (p. 68). This is the major theme of Donahoe et al., and I find its exposition extraordinarily liberating. The perspective is a kind of "unified field theory" in that, finally,

phenomena and principles that had hitherto been thought by many as distinct are brought together under a single selection rule. As the authors point out, this unifying principle had, in one form or another, been around since early Skinner, but somehow had become clouded by emphases on distinctions in procedures (e.g., roles of antecedents vs. consequences) as opposed to putative communalities in principles. I am reminded of a somewhat similar situation with electricity and magnetism in the 19th century. Maxwell brought together into one set of equations all the electromagnetic principles developed by Faraday, Ampere, Gauss, and others. This was no simple compilation, but a reformulation that yielded major insights; for example, that light is an oscillating electromagnetic field, which is one of the greatest discoveries in history.

In this commentary I focus on three issues. First, I will briefly explore some of the antecedents of the environment-behavior selection principle. Second, I want to speculate on the possible role of complexity theory in dealing with the molecular-molar distinction. Third, I will comment on the place of neural network models in behavior analysis.

#### *Antecedents*

The requirements for the conditioning of an environment-behavior relation was a major theme in the old continuity versus non-continuity battle that raged in the 1930s and 1940s with the Hull-Spence forces on one side and the Lashley-Wade forces on the other (see, e.g., Terrace, 1966). Donahoe et al. seem to be aligned with Hull (1929) when he said,

All the elements of a stimulus complex playing upon a sensorium of an organism at or near the time a response is evoked, tend themselves independently and indiscriminately to acquire the capacity to evoke the same response. For our present purposes the indiscriminateness

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of the tendency is particularly to be noticed. (p. 498, quoted by Terrace, 1966, pp. 274–275)

This view stood in contrast to that of Lashley and Wade (1946), who asserted that stimulus control did not exist without differential training. As with all such controversies, there was no clear resolution. Part of the problem was that there was no unambiguous way to control history. Some stimulus control is clearly preprogrammed by natural selection; much, of course, emerges from selection via reinforcement contingencies, differential or otherwise. Hull's assertion seemed to be focused on a specific stimulus, perhaps multi-dimensional; but in nondifferential training, neither the possible controlling stimuli nor their dimensions can be known without post-testing. Donahoe et al. would surely disagree with the "all elements of a stimulus complex" portion of Hull's assertion.

Rescorla (1967, 1988) and Rescorla and Wagner (1972) based their theories of Pavlovian conditioning on some measure of correlation between the occurrence of a conditioned stimulus (CS) and the occurrence of an unconditioned stimulus (US). In doing so, they did not make the important distinction emphasized by Donahoe et al., namely, "Contingency is the language of procedure; contiguity is the language of process" (p. 200). Actually, Rescorla and Wagner (1972) came close with their invocation of context in the analysis of acquisition. Thus, if the correlation between the CS and the US is zero, this means that the probability of the US given the CS plus context is equal to the probability of the US given the context alone. Given their assumption that the associative strengths of stimuli are additive, this leads to no associative strength accruing to the CS. This is a pure contiguity theory. Pavlovian conditioning wasn't what Rescorla (1988) thought it was either. That contextual conditioning can occur is well founded in, for example, the drug-tolerance experiments of Siegel (e.g., 1975).

A conditional probability description of operant conditioning could also be given that includes a sample space of reinforcement ( $S^R$ ), an operant class ( $R_o$ ), the context (CTX), and a potential discriminative stimulus ( $S^D$ ). For example, in nondifferential con-

ditioning, the  $S^D$  (say, a keylight) is actually part of the context, that is,  $S^D \subset \text{CTX}$ . Instead of a simple two-term contingency expressed as

$$P(S^R | R_o) \text{ versus } P(S^R | \overline{R_o}),$$

we have a contingency involving multiple terms, for example,

$$P(S^R | \{(R_o \cap S^D) \cup [R_o \cap (\text{CTX} - S^D)]\})$$

versus

$$P(S^R | \{(\overline{R_o} \cap S^D) \cup [(\overline{R_o} \cap (\text{CTX} - S^D))]\}).$$

Many other combinations are possible to specify arrangements for discriminative control, intermittent reinforcement, and so forth. In this example, the degree to which responding will occur in the given context will presumably be a function of the difference in the conditional probabilities. If the difference between the first and the second were positive, then responding is inextricably linked to the context in which it occurs as a result of response-correlated reinforcement. Again, the fundamental requirement is contiguity, the temporal linkage of context, behavior, and consequence.

The unbreakable interlinkage of these three entities has always been inherent in the three-term contingency (see, e.g., Marr, 1993), and from the perspective of Donahoe et al., this linkage is appropriately generalized. What this means is that there can be no functional independence of context, behavior, and consequence. A clear analogy exists with Newton's second law:  $F = m(dv/dt)$ . The three terms in this expression—force, mass, and acceleration—cannot be functionally defined independently of the others. What saves us from a trivial circularity in both the behavioral and mechanical domains is the empirical application or demonstration of the interrelations, when given specific initial and boundary conditions. These applications show extended generality over a great variety of situations, which is characteristic of a coherent account. Through such accounts we can predict the outcome of nondifferential reinforcement when we manipulate the formerly unchanging context, just as we can send a Voyager spacecraft to Neptune.

#### *From the Simple to the Complex*

Analogies between reinforcement contingencies and mechanics can be extended in

more interesting directions, as is inherent in the significance given by Donahoe et al. to relations of moment-to-moment events to molar outcomes (see also Marr, 1992, 1996). The question is how molar or global patterns emerge from local or temporally constrained events. The contingent interrelation of context, behavior, and reinforcer is, in fact, a nonlinear dynamical system. Under appropriate conditions, such systems can display remarkable complexity, even though the rules that specify the dynamics can be quite simple. There is a rapidly growing field, called complexity theory, that is devoted to the study of such "self-organizing" systems (see, e.g., Coveney & Highfield, 1995). There are numerous examples from autocatalytic reactions characteristic of cyclic biochemical pathways (e.g., the Krebs cycle) to reaction-diffusion systems possibly engendering embryological development, including coat-color patterns in animals (Murray, 1993), to mechanisms controlling the formation of snowflakes and the tertiary structure of proteins. There appear to be essential communalities to these kinds of dynamical systems: nonlinearity, interplays of positive and negative feedback, and a delicate balance of stability and instability. Behavioral contingencies also meet these requirements for complexity. Whether behavioral systems can be usefully modeled by complexity theory remains an open question. Some dynamical systems are irreducible. The N-body problem is one of them; there is no way to take it apart to consider how each separate body interacts with the others—interactive terms remain intractably interactive. As for contingency systems, unfortunately we have not advanced very far in actually writing down systems of coupled equations to determine if they could yield known molar patterns of behavior from the appropriate moment-to-moment dynamics. So-called neural network systems offer a possible alternative, and that approach is emphasized by Donahoe et al.

#### *Getting Off-On the Network*

Although I would like to debate in detail the assertion of the neural function plausibility of network models, the space allotted here is inadequate. Suffice it to say that in comparison with any proposed or known *actual* local neural circuits (never mind big chunks

of brain with perhaps billions of cells) in the cortex, the retina, the thalamus, the hippocampus, the cerebellum, and so forth, network models are a joke. Just as Woody Allen described *War and Peace* ("It's about Russia"), network models are "about the nervous system." Both statements are true, but there are significant elements missing (see, e.g., Shepherd, 1994, for an introduction to the complexities of real nervous systems). Moreover, our understanding of the neural mechanisms of learning is primitive, relative to what we know in terms of a functional analysis of behavior. Thus, is not clear just what a "plausible" neural network model means. Kehoe (1989) seemed to set the proper perspective when he said,

These models have been constrained only weakly by the known architecture and functioning of real nervous systems. . . . Stripped of their surplus meaning, connectionist models can be viewed as a class of quantitative models, albeit very elaborate models, subject to the conventional criteria for testing any model. (p. 427)

What perhaps is the important communality of the nervous system and a network model is that both are contiguity machines with feedback; that is, given a particular input–output circuit, the temporal conjunction of a pattern of inputs can result, through internal modifications, in a patterned output. Both the input and the output patterns have stochastic properties. Thus, the conditional probability expressions presented earlier describing possible contingencies are not static but dynamic—environment–behavior relations emerge from sampling the space of possibilities over time. Network models are thus dynamical systems with stochastic properties. Analytically, they are a kind of concatenation or coupling of nonlinear difference equations whose outputs are a function of discrepancies between nodal activities. This is the positive feedback aspect of the system. Because there are maxima (and minima) in nodal activities, the system has an attractor, that is, a semistable equilibrium. Difference-equation models of acquisition and other behavioral phenomena are common (e.g., Rescorla & Wagner, 1972), but network models are far more complex than a single or even several such equations. The model proposed

by Donahoe et al. is particularly sophisticated with its Hebbian-type rules and diffuse feedback arrangements. (Without perusing the Appendix in Donahoe, Burgos, & Palmer, 1993, I found the model difficult to understand as presented in Donahoe & Palmer, 1994, and in Donahoe et al.) Aside from issues of physiological relevance (which the authors have certainly tried to address), my concerns have to do with the properties of these models themselves. I count some nine free parameters in the present model, not including the constraints on the ranges of weights that determine the stochastic attractor states. How are the parameter values chosen? With so many parameters, why could not any data be simulated? Only simulated data were presented, so it is difficult to judge how well the simulation works except in the most general way.

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### UNITS OF ANALYSIS AND THE ENVIRONMENTAL CONTROL OF BEHAVIOR

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We will begin our commentary on Donahoe, Palmer, and Burgos' article with some

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history that bears directly on our perspective on the current debate. From its outset, the research program at the Shriver Center and before that at the Massachusetts General Hospital had as its primary mission to understand and help to ameliorate problems of individ-